LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Fertilisation of Yucca

IN NATURE, vol. xxii. pp. 606, 607, appears a letter to which my attention has only to-day been called—signed E. L. Layard—on the subject of the fertilisation of yuccas successfully introduced and cultivated in New Caledonia.

The writer shows himself to be under some misapprehension as to the generic characters and appearance of the insect which is generally credited with the fertilisation of these plants in their native country. The moth of the genus *Pronuba*, to which he refers, is not a "large moth having yellow under-wings." Although a common species belonging to the *Noctuidæ*, standing in our British lists under the genus *Triphæna* (Ochs), but included in Dr. Standinger's European Catalogue in the genus *Agrotis* (Ochs), is distinguished by the specific, not generic, name *pronuba* (Lin.), as well as by the characteristic appearance to which your correspondent evidently alludes.

The genus *Pronuba* (Riley) was founded for the reception of *Pronuba yuccasella* (Riley) (see *Proceedings* Acad. Sci. Missouri, ii. pp. 55, 333; *Report* Nox. Ins. Missouri, v. 151, vi. 131; *Canadian Entomologist*, iv. 182; Hayden's *Bulletin* of the U.S. Geological and Geographical Survey, iii. 121–141, &c.), which has also been described by Prof. Zeller in the *Verhandlungen der zoologisch-botanischen Gesellschaft in Wien*, 1873, vol. xxiii. pp. 232, 233, under the name *Tegeticula alba*.

This small white moth, of which some varieties have a few black dots on the fore-wings, belongs to the Lepidopterous group *Tineina* (Stn.), possibly to the family *Hyponomeutidæ*. Prof. Riley finds that the female, which has the basal joints of the maxillary palpi developed into a long curved tentacle furnished with spines, uses these appendages to collect and convey the pollen of the yucca to the tube of the stigma, which it could not otherwise reach; the eggs are then deposited, and the larva feeds upon the fruit, subsequently hibernating and becoming a pupa on the earth. It would be most interesting to ascertain whether *Pronuba yuccasella* (Riley) has been introduced with the yucca into New Caledonia, or whether any other insect, either indigenous or not indigenous to North America, has been found to take its place in carrying on the work of fertilisation. Prof. Riley considers the fact that yuccas introduced into the more northern portions of America have failed to produce seed may be attributed to the absence of *Pronuba*.

If Mr. Layard will direct his attention to this point he can scarcely fail to supply some valuable and instructive evidence bearing upon the subject. WALSINGHAM

Eaton House, Eaton Square, November 13

Skin Furrows of the Hand

ALLOW me to contribute the information in my possession in furtherance of the interesting study undertaken by your Japan correspondent (vol. xxii. p. 605).

I have been taking sign-manuals by means of finger-marks for now more than twenty years, and have introduced them for practical purposes in several ways in India with marked benefit.

The object has been to make all attempts at personation, or at repudiation of signatures, quite hopeless wherever this method is available.

(1) First I used it for pensioners whose vitality has been a distracting problem to Government in all countries. When I found all room for suspicion effectually removed here, I tried it on a larger scale in the several (2) registration offices under me, and here I had the satisfaction of seeing every official and legal agent connected with these offices confess that the use of these signatures lifted off the ugly cloud of suspiciousness which always hangs over such offices in India. It put a summary and absolute stop to the very idea of either personation or repudiation from the moment half a dozen men had made their marks and compared them together. (3) I next introduced them into the jail, where they were not un-needed. On commitment to jail each prisoner had to sign with his finger. Any official visitor to the jail after that could instantly satisfy himself of the identity of the man whom the jailor produced by requiring him to make a signature on the spot and comparing it with that which the books showed.

The ease with which the signature is taken and the hopelessness of either personation or repudiation are so great that I sincerely believe that the adoption of the practice in places and professions where such kinds of fraud are rife is a substantial benefit to morality.

I may add that by comparison of the signatures of persons now living with their signatures made twenty years ago, I have proved that that much time at least makes no such material change as to affect the utility of the plan.

For instance, if it were the practice on enlisting in the army to take (say) three signatures—one to stay with the regiment, one to go to the Horse Guards, and one to the police at Scotland Yard—I believe a very appreciable diminution of desertions could be brought about by the mere fact that identification was become simply a matter of reference to the records.

And supposing that there existed such a thing as a finger-mark of Roger Tichborne, the whole Orton imposture would have been exposed to the full satisfaction of the jury in a single sitting by requiring Orton to make his own mark for comparison.

The difference between the general character of the rugæ of Hindoos and of Europeans is as apparent as that between male and female signatures, but my inspection of several thousands has not led me to think that it will ever be practically safe to say of any single person's signature that it is a woman's, or a Hindoo's, or not a male European's. The conclusions of your correspondent seem, however, to indicate greater possibilities of certainty. In single families I find myself the widest varieties.

15, St. Giles, Oxford, November 13 W. J. HERSCHEL

P.S.—It would be particularly interesting to hear whether the Chinese have really used finger-marks *in this way*. Finger-dips (mere blots) are common in the East, as "marks."

The Aurora of the 3rd Instant

MR. E. DOWLEN has kindly communicated to me some particulars of the above as seen by him at Southport.

He first noticed the aurora at 6h. 50m. (it had however been visible before that time) as a greenish white glow on the north horizon. This gradually rose until 7h. 45m., when the top of the arch was estimated at two-thirds of the way up between the horizon and the Great Bear. It then gradually died out from the ends of the arch, and at 8h. 30m. had disappeared. During the time it was watched the following changes took place :—

From 7h. to 7h. 15m. it faded away from the eastern end until 7h. 30m., when nearly half the arch was gone. The western end then seemed to gather itself up somewhat, and to get brighter. After this the ends again lengthened out until 7h. 45m., when the whole began to fade away. At 7h. 25m. a narrow-arched band of black cloud concentric with the auroral arch was formed. It seemed to start from the ends, and meet over the middle point. At first this lay close upon the aurora. It then rose quickly, passed through the Great Bear, and vanished. It took about ten minutes to form, rise, and disappear.

Mr. Dowlen saw no streamers, but faint ones might have been present and escaped notice owing to adjacent gas-lamps. The aurora was at no time bright, and Mr. Dowlen doubts whether any beyond the green line would have been seen in the spectroscope.

The cloud formation detailed seems to me of considerable interest. J. RAND CAPRON

Guildown, November 19

Temperature of the Breath

THERE is no doubt that Dr. Roberts has discovered the true explanation of the phenomena that puzzled me and a good many others to whom I showed them. I have repeated Dr. Roberts's method of heating the enveloping material so as to expel all moisture from it, cooling it down to the temperature of the room and then breathing through it. In every case where I did so the thermometer showed a rise to 112° and upwards at the end of a minute; at the end of two minutes the index was pushed into the small bulb at the top, showing a temperature of about 116°. It is evident, therefore, that the high temperature observed is not the actual temperature of the breath, but is

caused by the caloric evolved by the transition of the aqueous vapour of the breath into the liquid or solid form.

Before seeing Dr. Roberts's explanation I referred the matter to the greatest living authority on heat, and he, after carefully repeating my experiments, was of opinion that the heat was produced by the compression of the air when forced through the material. Had he known of Dr. Roberts's simple but ingenious variation of the experiment there is no doubt he would have accepted Dr. Roberts's explanation. R. E. DUDGEON

November 18

THE following experiment may serve to supplement the observations of Dr. Roberts as to the cause of the high reading of a thermometer wrapped in a handkerchief and placed in the mouth. An ordinary non-registering thermometer was wrapped in about twelve folds of a dry linen handkerchief placed in the mouth, and the following readings taken at intervals of one minute:—Inspiration was effected through the nostrils, expiration through the handkerchief. The thermometer was in the mouth from the beginning to the end of the experiment. Temperature under the tongue before commencing, 37°·0 C. The reading of the thermometer wrapped as above described, one minute after introduction into the mouth, was 43°·0. At the end of the second minute, 44°·1, 3rd 42°·9, 4th 41°·2, 5th 39°·6, 6th 38°·2, 7th 37°·1, 8th 36°·9, 9th 36°·9, &c. After the experiment the temperature under the tongue was 37°·6. Capillarity is probably the chief cause of the rapid condensation of water, and the consequent liberation of heat in the dry fabric.

In connection with the above I may mention a schoolboy's trick, viz., gripping the arm of a schoolfellow with the teeth and breathing forcibly through his coat-sleeve. The sensation of heat thus produced is much greater than when the breath is allowed to impinge on the bare skin.

In conclusion, I must freely confess that Dr. Dudgeon completely upset my objection, as to compression of the bulb having anything to do with the high reading, by the experiments quoted in his last letter. F. J. M. P.

Coral Reefs and Islands

IN my letter on "Coral Reefs and Islands," published in NATURE, vol. xxii. p. 558, I have just noticed an important slip in writing which demands correction.

In the third paragraph and ninth line, for *metres* read *miles*, so that the passage shall read thus: "On the Florida coast we have barriers with channels 10–40 *miles* wide."

More accurately, the space between the southern coast of Florida and the line of Keys (old barrier reef) gradually widens from a few miles in its eastern to more than 40 miles in its western part. The channel between the line of Keys and the present reef is 6–7 miles wide and about 150 miles long.

Berkeley, California, November 2 JOSEPH LECONTE

Vox Angelica

I HAVE received a letter from Mr. Samuel Ray of Stoke Newington with reference to my remarks on the Vox Angelica stop on an Estey American organ. Mr. Ray informs me that Gordon's supplementary tuning-valve is used for the desired effects. The *rationale* of the method is, that by partly closing the mute the reeds are flattened, just as one reed is when the key is partially depressed. Mr. Ray also says, that by pulling out the stop a little way and making the reeds beat the latter are liable to be drawn out of tune; but this was the original method, but is now improved upon. A separate mute is placed on the top of the tubes, so that the wind strikes one of the sets of reeds vertically, whereby undue strain is avoided.

GEORGE RAYLEIGH VICARS

Woodville House, Rugby, November 18

Fascination (?)

PROBABLY none of your readers have thought it worth while to make any comment on the letters on this subject which have recently appeared, because it would seem needless to discuss the *origin* of "fascination" by means of the eye of a snake (or whatever may be the stimulus to the alleged condition) while all the evidence we can obtain from these reptiles in confinement proves that the condition does not exist. It devolves upon those who might object to observations on reptiles in a glass case as untrustworthy, to show us why—all their other actions being normal—the prisoners should not exhibit the same habit in respect to this "fascination," as they are alleged to practise when free. It is rather late in the year now; but if Mr. L. P. Gratacap will take the first opportunity of seeing snakes feed, and if any of your readers will pay a visit to the Zoological Gardens, both he and they will, I think, come to the conclusion that, beyond the expression of a little surprise (on the part of ducks and pigeons chiefly) which soon wears off at the sight of an *unfamiliar* object, both the birds and animals regard the snakes with marked unconcern. I have seen a guinea-pig, after finding no place of exit from the cage, quietly settle itself down in the midst of the coils of an Australian constrictor, shut its eyes and go to sleep. Ten minutes afterwards the snake had moved, and the guinea-pig was washing its face with its paws. Not once, but a dozen times, a rabbit has nibbled the nose of a River Jack viper (*V. rhinoceros*) in a pretty, inquiring way, heedless of the strong blows the reptile would administer with its snout to the impertinent investigator of that queer-looking object. For fully ten minutes one day a rabbit sat gazing at the poised and threatening head of a puff adder, now and then reaching forward to smell the reptile's nose, and anon sitting on its hind legs to wash its ears, and again returning to the "fascinating" object of its inquiries. If during that time the rabbit had fallen into the state of trance, it was so soon released from that condition as to be able to attend to its own comfort and busy itself about its toilet. The birds show no more recognition than the other animals of the dangerous position in which they are placed. We see them hopping about on the snakes and pecking lustily at their scales; sitting on the branches, preening their feathers and behaving themselves just as though no such dreadful (or pleasing?) sensation as "fascination" was possible!

I saw once a sparrow perched upon the body of a snake twisted round a branch, and preening itself. By-and-by a constrictor crept up slowly, touched the bird with its nose, and then threw the crushing folds around it. The deliberate approach of the snake and the unconscious attitude of the sparrow, concerned about its private affairs, would have staggered any ordinary believer in "fascination." I have closely watched the behaviour of snakes *intent* on feeding. It may be a sudden rush, when the victim has no time to see its enemy, or the gradual, lazy advance of the reptile; in either case the doomed victim betrays no suspicion of danger—at least so far as I have been able to ascertain after passing some hundreds of hours contemplating the snakes in the unequalled representative collection of the Zoological Society.

The expression in Mr. Gratacap's letter, "glittering" eyes, applied to the orbits of a snake, which are veiled by the "antocular" membrane, and capable of very slight movement, may remind us of Virgil's "Suffecti sanguine et igni," and help to confirm the "basilisk" (not a snake, by-the-by) superstition, but can only serve to perpetuate a myth. Whatever may be the value of Mr. Foot's opinion, I would ask, "Who has ever seen a snake 'raise its tail' after the manner of the cats?"

Charles Darwin has much to say on this subject to any one who chooses to consult the "Origin of Species." He does not see any advantage in the cat's "waving" tail or the noise of the "rattle" of *Crotalus*, for no predatory animal would derive any benefit from a signal of warning to its prey. The snake certainly never "waves" its tail when intent on mischief.

ARTHUR NICOLS

Soaring of Birds

REFERRING to NATURE, vol. xxiii. p. 10, may I suggest the following?—The question seems to be: "How can birds, having attained a certain elevation, thence rise without further muscular effort?" If I am not in error in what follows, they can theoretically do so if they *start* with a difference between their horizontal velocity and that of the wind, and *end* with a less difference; *e.g.*, if they start at rest with respect to the earth, and end by drifting with the wind entirely.

Take this last case, and consider the earth as plane, and the wind as horizontal, and having a velocity $= v$ with respect to the [earth and] bird. Finally we suppose the bird gains a horizontal momentum $= mv$. Then, by conservation of horizontal momentum, the only force acting being vertical, the air must lose an equal horizontal momentum.

Now we know that in all cases of bodies colliding and ultimately acquiring the same velocity, while we have conservation